BOYLSTON PRIZE ESSAY

FOR 1860.

By DAVID W. CHEEVER, M.D.

BOYLSTON MEDICAL COMMITTEE.

By an order adopted in 1826, the Secretary was directed to publish annually the following votes:—

1st.—That the Board do not consider themselves as approving the doctrines contained in any of the Dissertations to which premiums may be adjudged.

2d.—That in case of publication of a successful Dissertation, the author be considered as bound to print the above vote in connection therewith.

It will be noticed that the references are only of a general character. As the writer has not had access to any work bearing directly upon the subject, it has been his endeavor to unite the best views of Medical Logic and Philosophy into one chain of reasoning upon the question proposed. Particular references were not made at the time of writing, and it would be very difficult to recover them now; but wherever any considerable statements of facts, figures or argument are cited, they are referred to at the bottom of the page.

D. W. C.

THE VALUE AND THE FALLACY

OF

STATISTICS IN THE OBSERVATION OF DISEASE.

BY

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Perpendendæ, magis quam numerandæ, observationes.

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THE VALUE AND THE FALLACY OF STATISTICS IN THE OBSERVATION OF DISEASE.

"Je sais que la vérité est dans les choses, et non dans mon esprit qui les juge; et que moins je mets du mien dans les jugements qui j'en porte, plus je suis sur d'approcher de la vérité."—*Emile J. J. Rousseau.*

This saying of the sage of Geneva has been used as evidence for the value of statistics, but it contains, also, as it seems to us, the germ of their fallacy; since they tend, when carried too far, to separate reason from observation; to ignore the subjective, and to study only the objective phenomena of disease. They would lead the medical observer to overlook, with Rousseau, the fact that it is by his mind that he judges, and that the rectitude of his decision depends on his own mental acuteness.

To adopt a clumsy but expressive phrase of Carlyle: "The eye sees only that which it brings with it the power of seeing." Previous knowledge and particular training, as well as common sense, are necessary to the correct observation of any class of phenomena. Observation must always vary with the character of the observer. We are the standard by which we must judge of external nature. The true understanding of natural laws is never attained wholly from without, nor immediately from nature, but rather from our own conceptions and deductions. Our senses, of themselves, teach us nothing, but only furnish us with impressions from which to infer and by which to determine the actual state of things outside of ourselves. And these impressions must inevitably differ in each individual, in accordance with his own preconceived ideas of the nature or uses of the object he examines. The ancient Romans assembled as varied specimens of the Animal Kingdom in the great *vivaria* of the Flavian amphitheatre, as the Jardin des Plantes can number within its enclosures. And yet they derived from them, if any, but the rudest notions of their peculiarities, races, and modes of life; while the broader generalizations of natural history and comparative anatomy never once crossed their minds. Archimedes saw only in the convex lens an instrument to burn the enemy's fleet; but Torricelli, the revelation of a new world of microscopic nature.

Nor can numbers, any more than our senses, teach us anything by themselves. The mere observation of simultaneous or consecutive occurrences, however great the number of cases may be, can lead to no definite results which may not be fallacious. A causal connection must be otherwise established. By a neglect of this circumstance, observers have been betrayed into the most absurd conclusions. Some would regulate the periods of parturition by the changes of the moon, and make the births of our race coincide with the quarters of that satellite. Others, as Boudin, by computations, bring the motion of the earth about its axis and around the sun, into a certain relation with the weight and excretory processes of the human body, thus pretending to establish a tidal flow of our excretions, ruled over by remote planets: a supposition which might suggest to the astronomer that the next discovered member of the solar system be named Cloacina. It could be no more ridiculous for the stranger who passed the night in the steerage of an emigrant ship to ascribe the typhus, which he there contracted, to the vermin with which the bodies of the sick might be infested.

An adequate cause, one reasonable of itself, must correct the coincidences of simple experience; and the mere numeration of any series of events cannot establish any general principles without analysis, classification, comparison, and induction.

The term "statistics" is nearly equivalent to "political," or "social science"; and we prefer the use of the expression, "the numerical method," in medical *parlance*. This method has both a value and a fallacy: And our subject has been well stated in this respect, and wisely restricted to the *observation*, or natural history of disease. For it so happens that this is not only one of the most important branches of medical science, but one of the most difficult to define and accurately determine.

By the natural history of disease we mean the succession of phases which it exhibits when left to itself, uncomplicated by other morbid processes, and unmolested by active treatment. The wiser view of disease has gradually gained ground, that it is not a distinct entity, something extraneous to the usual processes of life, and inimical because foreign. But that disease is only a variation from health, a difference in the action of the normal functions of the body, yet only a modification of them. Hence the natural history of disease aims to exhibit all the morbid changes in the vital phenomena, whose sum constitutes sickness, or variation from health. We are led to believe, too, that diseases, in their course, have a natural rise, increase, intermittence, climax, decline and limit. The establishment of these facts; the self-limited nature of some diseases; the inherent tendency to recover in others, and the proportional mortality of diseases generally, when left to themselves, are of the first importance in determining the value of therapeutics.

Such knowledge is, from the nature of things, very difficult to acquire. The accumulated errors of the past, and the ever present obstacles of interest, prejudice and partiality, constantly impede our progress. It is by no means easy to find a proper field for such inquiry; and a true estimate of the powers of nature is interfered with through the officiousness of art. Most diseases are subjected to so active treatment, as must at once vitiate the result. The practitioner's own conscientious scruples against leaving any cases to the care of Nature alone, from the fear, magnified by his previous teaching, that he might be injuring his patients; the non-perception of the utility of the knowledge to be so acquired, and the dread of being exposed to the charge of malpractice, all operate against his obtaining a knowledge of the natural history of disease. Effects are ascribed to drugs which really flow from natural causes, and are but the usual succession of the morbid phenomena; sequences are taken for consequences, and all just conclusions confused.

From the want of this knowledge; from defective observation, rash generalizations, and hasty conclusions *à priori*, have arisen the thousand conflicting theories which have degraded Medicine from its true position as a science, and interfered with its advancement as a practical art. Soon after the decease of the great prototype of medical observers, Hippocrates, such sects as Dogmatics, Empirics, Methodists, Pneumatists and Eclectics, violated the principles which he laid down, and successively assumed the control of disease. Two of these, the Empirics and Eclectics, have survived the rest. Later, the Humorists and Solidists divided the medical world. Pharmacy next mounted the throne, and conducted a reign of indiscriminate drugging, in which the value of a prescription seemed measured by its length. Treatment oscillated between such extremes as the antimonial tolerance of Rasori and the bleeding *coup sur coup* of Bouillaud. Etiology, in the hands of the most opposite characters, was content to trace all maladies to a single cause; which *materies morbi* Broussais located in the alimentary canal, but Hahnemann upon the skin. Sydenham, Boërhaave and Hunter countenanced a return to the neglected rules of Hippocrates; and a tendency to more exactitude in observation, owing its inception mainly to the physical diagnoses of Laennec, culminated in the numerical method of Louis.

The unfortunate student of medicine has therefore ever been placed between the horns of this dilemma; either to try to reconcile these conflicting theories, or to mount the favorite hobby of the most enterprising spirit of his times. Not only has he had to pursue subjects of study, in themselves dry from their apparent want of connection, but he has, more than all else, been crushed by the multitude of isolated facts, simultaneously presented to his mind and memory, for mastery and retention.

How tempting, then, must the supposed certainty, the easy generalization and the all-embracing tabulations of the numerical method, have seemed to him! A way appears at last to have been opened out of his difficulties, and he, perhaps too eagerly, follows the most promising path.

Although Louis first applied the numerical method, in its strict sense, to the observation of disease, yet the numbering of cases is as old as Hippocrates, and it was used by every practitioner who *counted* the facts of his experience, since that time. Capt. John Graunt, of London, has the honor of being the first writer who ever directed the attention of the world to the comparative births and deaths of different cities, years, seasons and sexes, and to the comparative mortality of disease. This he did in a work published in London in 1662. He had much genius for observation, and was really the creator of the new science of statistics. But the name owes its origin to Achenwal, of Göttingen, who published a work in 1749, in which the term *scientia statistica* occurs for the first time. The use of numbers as a means of comparison, in this work, led to the strange mistake of regarding their employment as a new method. It is more particularly during the present century that an attempt has been made by numbers to give to many subjects the exactness of the physical sciences, and to compensate for the want of demonstration by averages, and the balance of probabilities. Statistics, says Mr. Buckle, although a recent science, have already done more to advance the cause of human knowledge, than all other methods of investigation put together. Quetelet stands, perhaps, at the head of modern statisticians. The wideness and variety of the application of statistics is only equalled by the varying degree of certainty which attaches to their results. Births and deaths, marriages and morals, accidents and crimes, are all averaged and tabulated, year by year. Diseases and causes of death; epidemics, hygienic and local surroundings; trades, professions and crops, are equally subjected to numerical analysis. And from this the curious fact of the average uniformity of all events results. Thus, male births are ever in excess of female in the proportion of about 21 to 20; marriages depend on crops and prosperity; disease favors certain districts in proportion to their hygienic state; fecundity succeeds epidemics; accidents do not exist, but the same yearly average of casualties and crimes prevails approximately at all times. So that one can always predict, with much approach to certainty, how many persons will steal, or murder, or commit suicide, and even by what modes, or weapons, in any given period of time. Some have even carried this so far as to assert that the same number of misdirected letters, in the average, pass through the London post office every year; thus proving that not even occasional absence of mind is purely the result of chance.

It is obvious that very different degrees of certainty must pre-

vail in the various kinds of statistics. Thus, commercial averages of imports or exports; financial statistics; or those of births and deaths, or of the crops—must be approximately more accurate, than those which relate to hygiene, to epidemics, or to casualties. And this, because the data in the former cases are few, simple and ascertainable; but in the latter, many, complicated and obscure. And here we shall find one of the cardinal distinctions to be drawn between ordinary statistics and the numerical method as applied to disease, as well as one of the many fallacies of the latter.

We fear that the mistake exists in the minds of some, of confounding together the laws and results of Physical signs with those of the Numerical method. It is true that the application of both began, so to speak, in the same country, and that they are nearly coeval; we are the more apt, therefore, to confound them, though the distinction between them is really wide, and should be always borne in mind.

Both these methods claim great accuracy; both also are apt to be carried too far. The one to the neglect of general, rational symptoms; the other to the tabulation of incomparable facts, and the drawing of illogical conclusions from too few, or too obscure *data.*

Man is incapable of observing all the phenomena of any event, but this is necessary to a perfect view of it. It is precisely the younger observers, the advocates of exact investigation, who think they see everything, and that they are the first to observe correctly.

Yet there is much practical wisdom acquired in life, on which men are obliged to depend, and which was never obtained by a formal, or mechanical process. Thus, a knowledge of human nature is not learned by counting, but by reasoning; by induction, and not by numeration. And statistics, like physical signs, when pushed to excess, may destroy that readiness and practical tact, which is of the most value in the treatment of urgent cases. Should one wait to tabulate and compare the symptoms with the mass of statistics previously acquired, the individual case would probably die, while waiting for relief. And it very unfortunately happens that the most sudden diseases have not the best established therapeutical laws. In surgery, no one would hesitate to compress a bleeding vessel; but in medicine, room is always left for a balance of probabilities; and the statistician would have to enforce a fatal delay before he could decide from numbers to bleed in apoplexy, or to vomit in croup.

While therefore mere observation and numbers, by themselves, are equally inadequate to unfold to us that most important of subjects, the natural history of disease, some further method must be sought for, than pure and naked experience. For all that we know by experience, or that which we express by an empirical law, is only a simple fact that certain effects and influences occur together,

without our being able to point out their mutual dependence. And as there are no such things as accidents, or exceptions to natural laws, so in the living body, as elsewhere, no effects can occur otherwise than as consequences of fixed causes. They seem accidental and exceptional only because they are unexpected, and their antecedents unknown. Therefore, although the causal connection of events and phenomena is in our science peculiarly hard to trace, it is still the most legitimate object of investigation.

Analysis, followed by synthesis, are the two auxiliary methods in our search for causal connections. Analysis must precede synthesis. We must follow up the stream of events to their tributaries and source. We must break up and search the groups of similar and related individual facts, and then apply their laws to aggregates. By a natural impulse we follow analysis by an attempt at generalization. And here experience, fallacious though it be, must confirm our conclusions, or they are valueless. It is equally necessary to correct generalization, that we should discover resemblances, and demonstrate an agreement in essential points, among the phenomena, or relationships which we are seeking to bring under the control of a single law. Thus we should fail to explain the different agencies of chlorine on colors and on odors, did we not know its affinity for hydrogen, which operates in either case.

Yet, without generalization, our experience is but a chaos of empirical facts. And it is in the science of medicine that this difficulty of separating single phenomena from the magic circle of the whole, or of judging of their complexities in aggregates, or of generalizing widely and wisely by real resemblances, essential agreements, and well-known effective causes, is the greatest obstacle to real advancement.

We venture to say that there is but one other science, that of meteorology, comparable to medicine in uncertainty, and in the number and variability of unknown influences, which control its results. Who is a true prophet of the weather, or of disease?

For example, what a variety of influences affect a single organ, the eye. The laws of vitality, the rules of optics, the unknown chemical changes which photograph on the retina the image of the object seen; and finally the equally unknown connection of sense and nerve, brain and perception, all coincide in the government, the regulation and the result of vision. All its lenses, curtains and tunics are endowed with life; all are nourished by constant changes, and all are connected with the machinery of the whole organism. The cornea transmits, the iris cuts off the rays; the humors refract, the lens focalizes and renders achromatic, and the retina receives the soft impression of the sun's beams. Secretions moisten its exposed surfaces, and soft ciliæ free it from dust. Muscular agencies move it in combined action; habit, mental influ-

ence, or some unknown law, regulate the harmony of the perceptions of both eyes, and replace the inverted image which the upright object makes, into its proper position in the sense itself.

"The structure of the human frame as much surpasses the most skilful work of man's hands, as its functions do the play of his most ingenious mechanism, and its products the results of his most refined chemistry. That which he knows, bears no proportion to that of which he is entirely ignorant, and what he sees, he sees but darkly. He seeks for causes, but they elude his search; the vital principle, which contains the solution of his difficulties, baffles him at every turn; he strives, as it were, to seize it by force, but the violence which he uses defeats itself, and the tortured body dies that it may preserve the secret of its life. Such, and so inscrutable is the body in health; disease surrounds it with new mysteries. Its structure passes through strange transformations; its functions undergo wonderful changes; a new chemistry presides over its secretions, and new principles seem to pervade its every part. Exposed from without to a thousand varying influences; subject within to innumerable changes; governed by a subtle principle which pervades every part, but seems to have no single centre of action; the tenement and instrument of a mind which it both obeys and governs—the human body forms, beyond all comparison, the most difficult, the most complicated study which offers itself to our choice. Our knowledge of such a structure must ever be imperfect, even if we confine our inquiries to a single, human being, or if every human being were the counterpart of every other, and external influences produced the same effects on all. But each human body differs from every other in outward form, in inward structure; in health, in disease; in the degree of influence of external things upon it; in the effects of food and remedies."

What wonder. then, that the careful medical logician should lay down as general laws: First, that, "In the living body, the same event or effect may occur in very different manners, under varying and dissimilar circumstances:" a fact which greatly complicates the investigation of casual connections. And, Second (because of the very varied influences in the development of vital phenomena), that, "In the living body no occurrence or effect ever takes place with perfect constancy and certainty, and always in the same manner; and that we cannot, for this reason, with perfect certainty, predict or expect any occurrence or effect whatever."

From these insuperable obstacles medical science has conducted its inquiries through two distinct stages, and is only partially within the borders of the third. These stages, according to M. Compte *(Philosophie Positive)* are, First, fiction and faith; Second, speculation; and Third, positive science. Anatomy, physiology, surgery, and morbid anatomy are either firmly established in, or have already entered upon the third stage. But the natural his-

tory of disease has long lingered in the first two eras, and is slowly emerging from them; while therapeutics, worst of all, remains, but for a more or less fallacious empiricism, as firmly rooted in fiction, faith and speculation as ever.

Medical science has always been in advance of medical art; and the progress of the latter does not correspond to the discoveries of the former. The power of healing is much inferior to the capacity of knowing; the treatment, to the diagnosis of disease. All the obstacles which arise to impede our progress in the inquiry into the causes, or the natural history of disease, assume a tenfold greater magnitude when we attempt to investigate their treatment. So, likewise, the objections to, and fallacies of the numerical method vary with the subjects for which it is consulted. Statistics of etiology and structural lesion are more reliable than those of pathology; and all of these, *à fortiori*, than those of therapeutics.

The question naturally arises whether the difficulty in medical investigations is inherent in the subject itself, or is due to faulty observations. Both causes probably concur in producing it. If the complexity and uncertainty of vital phenomena prevent a clear and comprehensive view of their relationships and laws, no less do the preconceived opinions and inaccurate modes of study of the observer stand in the way of the attainment of the truth. The former of these applies with more force to the medical science, than to the medical art; to a collection of principles, than to their practical application.

Originally a mere art, medicine took the form of a science when men began to collect, arrange and analyze individual instances, and to express what was common to them all in the form of propositions. The art which preceded the science differed greatly from the art which sprang from the science. The one was mere empiricism; the other, practical science. The art of medicine, as it now exists, is the offspring of science; and we can no longer regard individual facts in any other light than as suggestions to new inquiries, hints for practice, and solitary materials for the formation of theories. So, true observation is the union of thought and perception, which analyzes, arranges and classifies the facts collected. A simple employment of the senses is not observation; nor does the exercise of them constitute experience. In this sense, therefore, the science of medicine cannot be said to be wholly empirical.

It is impossible that we should arrive at a certain medical comprehension and science by mere observation, and the collection of so-called experiences and facts, of themselves; for this is only the first step. The savage, wandering in the forest, sleeping without shelter, and dependent on the wind or weather for power of locomotion and the means to reach his prey, has no doubt always

been a keen and constant observer of the heavenly bodies and the clouds during all the nights of his existence. But his experience, unaided by comparison and generalization, has never taught him the true magnitude, or motion, or relations of the stars, nor the simplest laws of meteorology, as laws, and not as successive and empirical facts. Nor would the physician ever obtain, by mere observation, whether by palpation, auscultation and percussion, or by autopsies and microscopical and chemical examinations, however minute, a true insight into the real condition of his patients.

These are all means of knowledge, but, unless aggregated and compared, they must remain only isolated, empirical facts. We cannot wholly separate theory from observation, even by substituting figures for words. We must be careful only that our theories are based on constant and recurring facts. Even the purest empirical writer on Medical science, styles that science, "*the phenomena of life, with their relationships, classified and arranged.*" And although anxious to have it understood that science is not the inductive or reasoning process added to facts, nor a theory built upon facts, yet he equally includes the classification and arrangement of phenomena, under his definition.

Such classification, dependent on the identity or similarity of certain groups of phenomena, and their dissimilarity to others, necessarily requires comparison and generalization. And although some exercise of invention or intuition may lead to useful discoveries, afterwards confirmed by experience, yet *à priori* conclusions are but too apt to be premature. Induction; reasoning *à posteriori*, which forms the distinctive feature of the Baconian philosophy, is the safer course in all sciences. Theorists have uniformly neglected it; but careful observers have attained their most important results by following its laws. By it, or by observation and *experiment*, that *vexatio naturæ*, as Bacon calls it, the physical sciences have alone advanced. So all other branches of knowledge, which are, from their nature, incapable of demonstration, must proceed by comparing observations, and analyzing experience, rather than by either *à priori* theories, or simple empiricism, to establish general principles, and natural laws. But what is called a law in natural science, differs somewhat from the popular use of this expression. A law of Nature is nothing else than a general expression of the conditions under which certain phenomena occur, so far as these conditions are known to us.

We cannot account for a phenomenon by referring it to a general law. The law, or generalization, is governed by the phenomena, and not the phenomena by the law. So, for the sake of illustration, the laws of grammar are the expression of the conditions under which certain words and expressions usually occur. And when we say that an expression is at variance with the laws of grammar, we mean only that it differs from the more general usage

which constitutes those laws. Good usage does not depend upon grammar, but grammar upon good (general) usage. A law of nature, also, has no cöercive power whatever: whenever we speak of phenomena as *governed* by law, we imply some higher *will.*

The common fallacy that "the exception proves the rule," while it may show our generalization to have been imperfect, really means only that the exception indicates by its rarity the existence of some more general mode of action, to which the majority of phenomena can be referred.

They, therefore, who seek to establish positive laws of disease by the accumulation of isolated facts of experience, succeed, it is true, in proportion to the breadth of their researches, but cannot, as long as their observations are finite, arrive at absolute laws. Particularly in the complex and vague field of medical experience, it is still more difficult to establish and verify general principles, or laws.

Another source of fallacy lies in the common acceptation of the word *cause.* Usually held to mean the power which originates any action, it logically includes also the conditions of its operation. Thus, heat, generating steam, is the power which moves the locomotive; and its machinery, the condition of its progress. Both would be called the cause of motion; though, properly speaking, one is a dynamical, the other a material cause. So, in the human body, vital force may be the power, and respiration the condition of life; or miasmata might be the cause, and intermittent fever the condition of disease. Too much care cannot be taken, in investigating that most important branch of science, the causal connection of empirical facts, to bear in mind this difference in the signification of the term *cause.*

It is obvious that the causal connection of isolated events in medicine is not to be reached by any single, simple process of experience or numbers. For the course of reasoning by the inductive process is, first, observation; second, comparison and generalization; and, third, the establishment of causal connection: and when we know *all* the conditions and modifying processes, we can establish a natural law. Therefore the observer needs, a mind to guide observation; observation to acquire facts; logic to estimate the value of observation; analysis to separate complex facts; analogy to compare them; statistics to average individual facts; the calculation of probabilities to define, as accurately as possible, the value of his results; and many aggregates, and almost infinite observations, to generalize laws.

Why medicine should fail in meeting our requirements, we can best consider by comparing it with the more exact sciences.*

The most perfect kind of human knowledge is that embraced by

* *Vide* "British and Foreign Medical Review."

the pure mathematics. What are called the primary existences, as space, time and number, we cannot conceive *not* to be. Not only are they necessary existences, but all men who think about them must get very nearly the same notions concerning them. The measure of space, time, quantity and magnitude is number: number has the same meaning for all mankind, and it is the only thing about which difference of opinion is impossible. Numbers being independent of the things counted, and free from the errors of language, we can reason upon them, as we can upon nothing else. Arithmetic, Algebra and Geometry, and the whole of the pure mathematics, being sciences of quantity, and hence of numbers, and being freed by their symbolic expression from the errors of language and the errors of sense, are, as far as they go, perfect sciences. Arithmetic and algebra prepare, by their numbers, instruments of calculation for the use of the other sciences. The simplest use of these instruments of calculation is in the science of astronomy; which is eminently a science of numerical relations, as well as of pure observation. Calculation is the secret of the success of this science, and also of statics, dynamics and optics. These are all sciences treating of those simple relations and properties of matter which admit of being represented by lines, figures or symbols. The observations on which these sciences are based are of the simplest kind; and they offer the example of sciences of observation not much inferior to the pure mathematics in accuracy. Yet fallacies exist even in these sciences. In astronomical observations the senses are fallible, and the nicest instruments admit of error. But the astronomer rectifies his errors by multiplying his observations: he has recourse to the numerical method, and takes the *mean* of as many observations as he can procure.

Chemistry is a science which makes extensive use of calculation, but is still more dependent on the exercise of the senses, since the properties of matter which it investigates are more complex and obscure than those of the heavenly bodies. The chemist, however, has the powerful aid of experiment; for he can create and re-create almost at will. He can make the objects of his investigation identical; he need determine them but once; he can use them also to determine others. But figures pervade the whole science, and all its statements are made in the numerical form.

A numerical theory—the law of definite proportions—converted the art of chemistry into a science: a position which it might not have reached by the mere questioning of nature—by experiment alone. The chemist, like the astronomer, has to multiply his observations and experiments to avoid error. Calculation, and his power of making the matter he employs identical with that which he has already ascertained, give perfection to his science. He is capable of synthesis, as well as analysis. But when he reaches the *organic* world, synthesis fails. And here we have the first sign of

the uncertainty, the first shadow of the difficulty, which awaits the observer at the threshold of *Life.*

The mechanical properties of matter are investigated with as much zeal as its atomic affinities. A knowledge of them can be obtained only by repeated experiment, because they resemble each other, but are not identical. The mechanic, therefore, has recourse also to the numerical method: to ascertain the strength, or the durability of wood, iron or stone, he must take the mean of many trials.

Astronomy and chemistry, the one a science of observation, the other of experiment, owe their perfection mainly to calculation. The former is the more perfect, since it deals with simpler relations of matter, and depends less upon the exercise of the senses, than the latter. But the latter owes its superiority over mechanics to the power which it possesses of making the objects and results of its experiments identical with those which it has already ascertained, and which bear the same name.

The certainty of a science seems, therefore, mainly to depend upon the extent to which it admits of the application of numbers and calculation, or of the numerical method, to rectify its errors.

It is not surprising, then, that much should have been hoped from its employment in the notoriously uncertain science of medicine, but only that it should not have been earlier applied.

The imperfections of medicine as a science are inherent in the subject itself.

"The physician, unlike the mathematician, is not the creator of his own science; unlike the astronomer, he has no simple relations of matter to deal with; he cannot, like the chemist, make any two things which he examines or uses identical; the objects of his study are more variable than the winds and tides; and the materials with which he works infinitely more difficult to adapt to their uses than the matter which the mechanic or engineer presses into his service. In all his preliminary studies (with the exception of inorganic chemistry), in all his original inquiries, in all his practical applications, he encounters the varying effects and complicated phenomena of Life."

True observation, or the union of reason and perception, the diligent use of the senses, not ignorantly or empirically, but knowingly and with a plan, can alone afford us any hope of penetrating to the arcana of the living functions in health and disease.

Medicine is a science rather of observation than experiment: for the latter method is impossible in the natural history of disease, and of very limited and doubtful application in the therapeutical art. We cannot isolate individual influences from the magic circle of the functions of the body; nor thus place them in a distinct and favorable light for single, or repeated experiments. Were this possible, we should still be under the influence of one-sided and special views, and be in danger of seeking for that only which we wished. It is doubtful, moreover, whether we should be much the wiser, even if we could produce artificial diseases, or ex-

periment with drugs: for the causes would be so complicated and numerous, that we could not judge of the effect of each.

As observation is, then, from necessity, our chief method of acquiring medical knowledge, we might expect numbers, which express only aggregated, classified or generalized observations, to be of much aid to us in our pursuit.

After these necessary preliminaries, which have taken up so much space, have been duly considered, we have found by their statements, that the senses alone, and numbers by themselves, can do nothing; that the natural history of disease, though the most important part of medical knowledge, is the hardest to obtain, while the want of it has given rise to conflicting theories; and that the student of medicine is oppressed by the multitude of isolated facts. We have glanced at the origin and history of the numerical method, and at its wide employment now in various sciences; and have shown that experience alone will not serve us, but that we must look for the causal connection of events: and while they are to be sought by analysis and synthesis, yet since generalization is peculiarly difficult in medicine, we finally must agree with the logician, that no effect or event can be predicted with certainty in our science or art. We have shown why the science of medicine is superior to the art; that the numerical method is even more inaccurate in the latter than in the former: also, that medicine, not being entirely empirical, we must classify and arrange our observations. We have endeavored to explain why induction is the only true way to arrive at laws; and to prove, by the true definition of the term law, why finite observation cannot establish absolute ones. And having shown that various mental processes, as, logic, analogy, comparison, &c., were needed in true observation, we have lastly compared medicine with the exact sciences: and inferred, from its great advantage in them, that the numerical method should be tried in the science of medicine.

We are now ready, therefore, to judge of the value and the fallacy of statistics in the observation of disease. It will be more convenient to reserve their value for our final consideration, and we will first treat of the *Fallacies* of the numerical method, as applied to the observation of disease.

Such fallacies may, in the nature of things, exist either in the object, or the observer; may depend on some inherent defect in the thing observed, or upon defective observation on the part of the person observing. And first, with regard to the fallacies in the object.

The value of our observations depends on both their quality and their number. The most positive results in any science are attained just in proportion as the facts compared, which form the basis of the statistics, are identical. Identity of facts, submitted to the numerical method, cannot be found in medicine,

No series of phenomena constituting disease, whether occurring in different patients, or in the same patient at different times, can be identical. Even the most exact of symptoms, those included under the head of Physical Signs, cannot be expressed by symbols, or figures; nor are their relations so simple, nor their analysis so exhaustive and complete, as to warrant their being always considered *the same.* Although the numerical method is not a mere substitute of figures for words, yet its results will always be accurate in proportion to its approximation to figures, which are ever one and the same. No diagnosis can be so minute, no malady so defined, no human body such a machine, as to give identical results. Therefore, not only the demonstration of pure mathematics, but the identical certainty of experimental chemistry, must be foregone in medical science. But if no identical units of observation can be obtained, our next care must be to obtain those which are strictly *comparable;* for without this condition is fulfilled, our conclusions will be not only inexact, but of no value whatever. There is, first of all, great room for error in the selection of our cases, and in the circumstances under our control. The disease must be well defined and carefully diagnosed; and in this respect, acute are much preferable to chronic maladies, for the latter are, at best, of very difficult classification. It is obvious that cases of a different nature may be confounded together, and improperly compared in statistics. The symptoms are of little value without a just notion of the cause of the disease, and the real nosological whole which the entire group of symptoms represents. Thus, a bellows murmur in the heart may depend on a peculiar constitutional disease, as rheumatism; on ossification of the valves, from age; on disease of the lungs, reacting on the circulating organ, or on some peculiar state of the blood itself. Therefore it would not do to compare, for the purpose of tabulating and averaging, every case which presented a valvular murmur in the cardiac region, without a knowledge of other concomitant disease, or the nature of the cause.

The patients must also be placed under the same circumstances of attention, diet and treatment, if the observations are to be continued through the natural history of the disease. But there are many circumstances beyond the control of the physician.

It is not, however, absolutely necessary that those things beyond our control should be included in the laws of comparable cases, nor that the want of them should invalidate our conclusions. It is true that we shall not get strictly comparable facts without them. But since the variability is known and limited, we can remedy it, in a measure, by the application of the numerical method.

But this requires the application of very large numbers of observations. This is well exemplified by the state of Life Insurance Companies in America. Known causes of disease and laws

of vitality are carefully collected and compared, but a vast number of unknown ones yet remain beyond control. To remedy these errors, the number of facts must be enormous, and extend over a long period of time. A sufficient number of years have not yet elapsed, since our country became populous, to furnish facts enough. And the result is, that the mortuary statistics of American Life Insurance Companies are far inferior in reliability and exactness to those of the older countries; and hence, as a secondary consequence, the profits and success of such companies are far more precarious here than there.

So much is this the case in medical statistics, that the more modern advocates of the numerical method charge even Louis with having based his conclusions on too few facts. The application of very large numbers to the calculation of probabilities, we shall consider further on.

The larger the number of cases, the nearer we approach to demonstration. And to secure the greatest comparability, with the fewest sources of external error, and the best chance to correct them by multiplication, we must employ "the simplest cases of a similar kind in the smallest possible field; and the same kind of effects under the greatest variety of circumstances; and, finally, collect and compare the results."

Besides even the impossibility of procuring identical facts, or the chance of strictly comparable cases, we shall find another source of error, even in those cases which are comparable, in the individuality of each case. Patients are not bundles of symptoms, nor are diseases always bounded by the same formal laws. And it has been well said that we cannot weigh a cough, measure the exact extent of a pain, nor determine fever by the atomic theory. Idiosyncrasy, or the peculiarities of the individual, are as anomalous and impossible to reduce to rule and measure, as the passage of the clouds. Nor are they infrequent. While similarity is frequent, there is usually enough unlikeness for comparison. Two cases of disease are as rarely identical in all their features, as are the faces of the patients. No depth of observation, no accuracy of numbers, no vastness of tables, and no grasp of memory, will ever enable the practical physician to reduce the case before him to real rule and measure, or to dispense with the necessity of considering each patient by and for himself.

The numerical method affords us a numerical estimate of probability in a given number of cases; but this is not of much help to the practitioner at the bed-side, who has to determine the probabilities of the individual case before him, which may or *may not* be more or less similar to the cases estimated numerically. Perception, comparison and deduction are necessary for each individual, as well as in formulæ and tables. We may often learn from statistics, indeed, that of many effects or consequences, as the duration,

course or result of a disease, one will occur more frequently under given circumstances than another. All this, however, admits of no direct application to an individual case. So many special circumstances and influences come into play in each case, and complicate the question and calculation to such an extent, that the conclusion cannot be a safe one. Each individual case has its own special result, which could not be calculated exactly from statistics. If we know, for instance, that of those attacked by cholera, fifty per cent. die, and an equal number recover, we cannot, on that account, assume for an individual cholera patient, that the probability of his dying is precisely as great as that of his recovery. Such probability must be greater or less than the average, according to his age, habits, personal vigor, or the severity of the attack. Nor, again, if we knew that the mortality of smallpox was just one in four, could we predict for any given case of this disease, that his chance was just one in four. It would be either above, or below that number.

There is still another important source of error existing in each individual case; allied to, but in some senses distinct from, idiosyncrasy. This is the "constitution" of the patient, as it is called; by which is meant the sum of all the influences of locality, station, hygiene, occupation, habit, diet, or accident, which have acted upon the individual from the time of his birth, until the period of the disease we are treating. And not only this, but influences called hereditary, in some diseases of great gravity, which extend back through generations before his birth. It is plain that we can never expect to attain a knowledge of all these influences by the most detailed examination or the largest tabulations; yet such knowledge is indispensable to render our facts, or units of calculation, not identical, but *strictly* comparable. It is, after all, with much justice, that following the popular idea, we hear patients say, "I value my family physician because he understands my constitution": and so, to the extent permitted by his finite faculties, he does.

The positiveness of principles (and principles are only generalizations), says an empirical writer, applies only to the principles themselves, and not to the individual phenomena and relationships, by the aggregate of which they are composed. This applies to both pathology and therapeutics. The positiveness of the law cannot apply to single instances. So diagnosis, prognosis and treatment must ever depend mainly on an accurate knowledge of the individual cases themselves.

To avoid as many as possible of these errors, to compare justly the essential points of one of those great nosological groups of symptoms which constitute a single disease, how many observations, and how many separate comparisons must be necessary. Each symptom, essential or accidental; each sensation, positive or

negative, present or wanting; every organ, as the skin, the tongue, the heart and lungs, as well as the more general functions indicated by the pulse, respiration and excretions, and, finally, *all* the functions, whether morbidly affected or not, must have tables, subtables, columns and balances by themselves. So, too, must the previous history of the patient be detailed with a fidelity which would require a mind and memory far beyond the average of humanity, especially when sick; and the inquiry into so many minute symptoms demands a patience, an accuracy, and a power of differential diagnosis, such as very few of the best trained physicians possess. It would be impossible to find cases strictly comparable in all these respects. The presence of any one symptom cannot be offset by the absence of another; nor even in strictly identical symptoms can those either wanting, or in excess, be mutually balanced, unless by numbers extending over vast series of cases, and a very considerable length of time. It is plain that the busy practitioner can never find time to work out such numerical results for himself, but must depend upon the labor of others.

Not only does this independence apply to different classes of maladies, and to individual cases, but we shall find, by pushing our inquiries more into generalities, that no department of medical science follows from any other, but that each is distinct by itself. A knowledge of one branch of medicine cannot be deduced from any other, but each also must be studied by itself. Thus, our knowledge of anatomy is distinctly the result of pure observation. Physiology is not deducible from anatomy, any farther than we can trace a general adaptation of means to ends; as that the skull is made to contain, or the heart to circulate something, but what, its uses or functions, we know not.

Neither is pathology deducible from physiology, though the common opinion is just the reverse. We cannot infer the action of a function in disease, from its normal action in health; as, for instance, the excretion of urea by the kidneys in health, but its retention, and the excretion of albumen, a constituent of the blood, in Bright's disease. Physiology is useful to pathology only as a standard of comparison; and *so* it is very necessary, in judging of the comparability of many units and data of statistics. Etiology, too, does not follow from pathology, but is the result of observation alone. And finally, therapeutics are not founded so much on pathology, as on simple experience. All this must greatly increase the labor of the numerical method.

And since pathology is not founded on physiology, it follows that the action of medicines in disease cannot be safely inferred from their action in health. Therapeutics establish the relations between articles of the materia medica and certain morbid processes, not healthy ones. To a limited degree we have exceptions, as where an emetic substance would empty the stomach of a

well, as quickly as of a sick person. But opium would often have far different effects in health or disease; pain alone being a perfect antidote to its poisonous action: and other instances might be cited. This does not interfere with the accuracy and value of the researches of Lehmann, and Bidder and Schmidt, on the physiological action of water, coffee, alcohol, or mercury; but should be a caution to us not to infer too readily, that the same results will follow their employment in disease, as in health. We wish only to show that the numerical method, when carried into pathology and therapeutics, meets only with increasing uncertainty and labor, and can in no wise be aided by a knowledge of the more rudimentary branches of medical science, which may have been earlier acquired. Everything tends to prove that not only each person and each branch of medicine as a science or an art, has an individuality which cannot be readily compared to others: but that diseases, especially, admit of infinite variety in the degree, the number and the order of their symptoms.

Yet for the correct study of disease, an arrangement of symptoms into groups is important, on the one hand, and a searching into the intimate nature and causes of such groups, on the other. Otherwise we are in danger of erecting symptoms of many diseases into diseases themselves, while in fact they are only symptoms. And in popular speech we often commit this error, calling the most prominent symptom, as diarrhœa or hæmorrhage, the disease. After we have successfully accomplished these nosological generalizations, we shall still find nature too complex for single observation, or numerical analysis. For our symptoms must be studied one by one, in the order of their development, and the precise period of appearance of each must be learned. One is apt to be confused by the multitude of symptoms which a patient presents. It is here that we discover the true value of hypothesis; not to analyze or theorize, but to enable us to isolate certain symptoms, previous to observation. Here, too, when it is practicable, experiment would come to our aid, because by it we can place certain symptoms in the best position for examination, thus judging of individual influences; and, in the physical sciences, we can repeat the experiments as often as we need. Fallacious grouping and the complexity of nature offer, therefore, serious obstacles to the correct analyses of the numerical method; while experiment is seldom admissible.

So many forces are at work modifying each other in life that correct induction is difficult, and the establishment of causal connection, at times, impossible. Vital phenomena move in curves, and not in straight lines: the non-appreciation of any one of the motive forces destroys the balance of antagonism, and would be fatal to the most careful calculations. Even the student of the physical sciences would find it difficult to ascertain the laws of motion, solely from bodies kept in a state of rest by opposing forces.

Again, the natural sequence of morbid phenomena is another source of error in establishing by numbers the natural history of disease. Not knowing the whole chain of causal connection, we are unable to decide whether one event takes place in consequence of a stage of the disease arriving to determine it, or from some other cause, which we have under our control.

Thus, too, the very different influences under which patients may be placed, were they only so seemingly trivial as the temperature or the state of the atmosphere, have a modifying effect on each case, which we should seek in vain to reckon or account for. Time and place even may render the statistics of different epochs, or localities, wholly valueless for comparison with others. The season of the year, the tendency of the epidemic then prevalent, the very various effects of good or bad locality, in a hygienic point of view, may all concur to derange the true balance of calculation.

We are in danger also of forgetting that it is not alone the number, but the intensity also of the symptoms, which are to be the units of our statistics. But the latter cannot be accurately measured, nor expressed in numbers.

We have already adverted to the necessity of very large numbers in determining vital phenomena: the true use of the calculus of probabilities, and the approximative accuracy of which it is capable, we prefer speaking of under the head of the value of statistics, towards the close of this article.

We come now, then, to the crowning fallacy, among the *objective* ones, of the numerical method, namely, the influence of the Vital Principle.

Medicine can never be an exact science, since it deals with the vital principle—a principle in itself changeable, self-supporting, and self-regulating. Vital force is the great perturbative element which renders the results attained by the faithful student of Nature approximate rather than precise. The calm test of experiment and the pure logic of analysis in organic chemistry are rendered uncertain, through an imperfect knowledge of its laws. Chemical processes, which, duly carried out under similar circumstances, give always the same result in the laboratory, are often wholly and inexplicably changed in the living organism. It is very evident, then, how it must disturb the results of pathological statistics; nor does there seem to be much probability of its being finally fully understood.

Liebig, on the one hand, and a modern and very voluminous medical writer of this country, on the other, represent the two extreme views held regarding it. The former would reduce it to a physical, if partially unknown, phenomenon; the latter would exalt it into a position of complete supremacy over all merely physical laws. Both, probably, equally err. The second named

gentleman has been betrayed by his enthusiasm into a denial of all chemical theories as applied to the human frame; of all discoveries of the microscope, and asserts, even, that the lacteal absorbents have open mouths. Liebig endeavors to bring all vital manifestations under a few universal laws of chemical affinity, and flushed by his discovery of the interchange of oxygen and carbon in the capillaries, and its connection with animal heat, hastens to construct a theory of calorifacient and plastic foods, and to draw close lines of comparison between vitality and some forms of electricity; conclusions, to say the least, premature. To his views Carpenter inclines; but Paget and Dalton are disposed to view the vital principle as a higher law. Carpenter describes vital force as a power correlative with the physical forces; as, for instance, that it may be the mode of action of heat or electricity in the body; and that physical and vital forces mutually give rise to each other. Paget says that vital force is distinguished from all others by its powers of generating typical organic forms, or *modality*. Though correlated with the physical forces, it has no identity with them.

The distinction is clear between the force, which by chemical action prepares the material for constructing an organ, and the force which forms the ideal plan of the organ, and constructs it of this material. Although the phenomena of living beings cannot be accurately analyzed, yet the two antagonistic forces are the organic typal, or modal force, creative and preservative of the organic form; and the chemical forces of the material molecules, that keep the substances of the forms in endless change. To take for illustration the phenomena of cell-growth. Mechanical force governs the position, shape and relations of the cell; chemical force governs its composition; but vital force assimilates it to the organ of which it is to form part, and gives it the power to partake actively of the vital processes.

It would seem, if further argument were needed, that the simple fact of the abrupt cessation of the chemist's power of synthesis, at the verge of organic life, were enough to prove that vitality was distinct from physical laws.

Although it is true that science has gradually brought some phenomena of digestion, heat and nutrition under the dominion of chemical laws, yet the total failure to subject the nervous element, and its connection with mind, to any sufficient physical or material hypothesis, would also incline us to believe that there is indeed a higher law of life, inscrutable, omnipotent and omnipresent in the human organism, and that that power is vital force.

Such being the sum of our possible acquaintance with *vital force,* it is obvious that we have present in all our formulæ of vital phenomena, in health or disease, an UNKNOWN ELEMENT, which no algebra or calculation can resolve, and which must at once vitiate the results of the numerical method.

But were all these fallacies of the object explained, or done away with, we should still have the liability to serious errors in the subject, or the observer himself. And first and most important of these is the influence of his own mind (the *ego*, as metaphysicians call it) upon the results of his observations.

The power of correct observation is innate: hence all observers are not equally good. The facts, then, of various observations will not be alike, nor alike reliable: but there can be no just comparison of dissimilar or doubtful facts. We never separate the object from the perception; it is impossible to do so. What we call objects are, then, our ideas of them: and our ideas will vary according to our preconceived notions, and our judgment. These observations are not comparable. Probably it is through our judgment that we err oftenest. We are too credulous of notions which agree with our own, and too skeptical of the contrary. Everything that is novel irresistibly attracts some persons. In medical science we are ever ready, like children, to follow any authority.

When we express our own explanation of what we have observed, compared and grouped with many other things, we describe it as it appears to us. For this reason our whole previous knowledge and experience, our notions and views at the moment, acquire a much greater influence over the mass of our observations, at the bed-side, than we are conscious of. We are continually liable, in confounding our own notions with our observations, to fall into the same error as metaphysicians, who have been unable to solve their own problems, because the mind observing was the mind observed; the mental faculties were both the instrument and the object of investigation. These philosophers were obliged to employ a fallible insight to detect fallacies in their own minds.

It is plain that our tables of cases are filled with the vagaries of as many minds as there are observers. We cannot guard against their mistakes. Everything that suits us, assumes such a magnitude in our eyes, that it causes us to overlook many other important details. A deficient experience and youth formerly inclined us more to theorize from our observations. Maturity is a safe age for the observer and collector of empirical facts. But, at any age, we are too apt to make rash generalizations. The very talents of an individual with any peculiar tendency to explore only certain subjects, become injurious to the medical profession by establishing premature and special, or limited, theories.

Specialists are particularly prone to error. The advantages to medical knowledge of so many minute observers, each investigating a single subject, is more than counterbalanced by their unconscious tendency to distort facts to suit their theories. And if it be said that there can be no difference of opinion about figures, we can easily point to a hundred instances in medical history,

where the unitary symptoms which are the basis of statistics have been misconceived, preconceived, or mis-stated, by honest, but prejudiced observers. Even if we are not specialists, we, very few of us, escape many years in practice without contracting some bias, which would cloud the clearness and accuracy of our perceptions. No man is wholly free from it. And provided that we hold our wayward tendencies sufficiently under control to prevent our being led into any positive fallacies in observation, we are still in danger of not finding, or of overlooking, the most essential points in a case, and of dwelling upon those which are less characteristic and important. Thus two observers, taken from the number of general practitioners, may render a very different account of the same case. It may be objected that such errors as these are inseparable from any human investigation. But it must be recollected that we are trying by statistics to free our art from fallacy; and that the science of numbers stands or falls with the identity or comparability of the facts observed, or with the reverse.

Provided, however, that we escape all these sources of error, another awaits us in the use of language and definition. It is not true that the numerical method is a substitution of figures for words. Numbers alone can never supply the requisites of articulate speech. And, unfortunately, the language we employ is a very defective one. The terminology of medicine is very far from definite, because originally formed by ignorant men: yet we cannot avoid using it. The literal meaning of technical terms is no longer their correct one. False expressions, too, influence our ideas, in the end. Our idea of a disease is got from a set of symptoms described and grouped by words. Yet the latter, as well as our definitions, change with time.

It is as important, too, in establishing theories from facts, that our classifications should be identical or similar, as that our definitions should be accurate. Diseases must be classified on necessary and general conditions, not on variable and local ones. This is difficult, since the species of disease, its seat, lesion, or even each particular instance of it, all differ from the others. We are apt to be misled by false analogies. And even when we seek for what we deem the soundest basis, we shall find anatomical changes, as the foundation of our classification, of but little value to the practical physician, since attention is not directed to those points of pathology which are most important to him.

It will be unnecessary to prove that diagnosis must be, we may say, *the* most important element in the numerical method. Difference or error, here, is necessarily fatal. Yet, this is one of the most difficult parts of medicine. The power of minute and differential diagnosis varies much, not only with the knowledge, but also with the mental acumen of the observer. The observers of facts which are to form bases of calculation, need peculiar care

and caution. "A corps of trained observers is needed—trained in the same school—so that they may observe alike; then their observations, whether right or wrong, will be alike right and wrong." That is, they will be truly comparable, even if fallacious.

We shall find that observers have varied much at different medical eras. "Every one is, and remains a child of his time." It is impossible for the medical observer, any more than any other person, to escape the cotemporary influences of his age. We are insensibly affected by the views of those around us, in spite of ourselves. The theories prevailing; the erratic views of some innovating genius, who may have escaped somewhat from the above influences; the re-actions constantly taking place in opinion in consequence of previous errors in doctrine—all must warp and influence the medical observer. The pupils of Broussais, Laennec, Bouillaud or Hahnemann, will, and must *see* different things in their patients, as well as pursue different treatment.

Besides this, if we take the same observer, we shall find that some circumstances always make a stronger impression on him than others. The rarer events will even lead him to overlook the more frequent; because the former will attract his notice by their infrequency. Yet the constant repetition of the latter renders them much the more important for practical purposes. Thus a positive always impresses us more than a negative fact; the occurrence of a certain symptom in five cases, more than its absence in twenty; a cure, more than a failure. Some unhoped-for success attending a remedy firmly associates it, in our minds, with a disease as a specific, while it is really only a coincidence. "Extraordinary and interesting cases are always remembered." Yet great caution should be used in admitting them to much consideration in our experience. It has been adduced as a striking proof of the fallacy of simple experience, and the value of the numerical method (or counted experience), that Louis himself found that every *à priori* conclusion, which he had formed in his own mind from his experience—or the recollection of his own facts—when submitted to arithmetical analysis, proved to be erroneous. This proves, indeed, the fallacy of memory, but it does not prove the truth of figures based on experience; for the errors may exist in the observation, as well as the recollection of facts, as we have already shown.

Not only a capacious intellect, but very great shrewdness is necessary for the correct observation of disease. Those facts which are to form the units of averages, should be culled with peculiar care. Leading questions must be strictly avoided; and negative, collected with the same industry as positive facts. For organs, even *structurally* modified, sometimes give rise to no symptoms; and this should impress us with the necessity of noticing all the functions in our examinations. It must be recollected, however,

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that while all these things are essential to a correct employment of the numerical method, few observers are equal to carrying them out. In particular, will positive impress more than negative facts. Very few would have the shrewdness ascribed to an ancient Grecian, who, on seeing the votive tablets suspended in the temple of Poseidon by those saved from shipwreck, asked also for the names of those who had been drowned in spite of their vows.

What is true of one place may not be true of another, with regard to disease; locality admits of but loose connection with morbid processes. It is difficult to compare different epidemics justly; it is much more difficult to estimate the comparative prevalence of chronic disease, if one searches for its etiology in the influence of place—In seeking for a comparative view of phthisis, for instance, as it extends, more or less actively, in many neighboring towns, or counties.

We cannot justly conclude that the seaboard, or inland townships are the more exempt from, or that high or low, dry or moist localities favor the ravages of tubercle, on so simple data as the mortuary statistics of the several places, in this respect. The number of deaths from consumption in a certain town, or even parts of a town, for a few years, does not prove much, positively, with regard to the influence of the locality on the disease. And this, because there are other more important causes, predisposing or exciting, of tubercle, which are ignored in the inquiry. Such are, hereditary taint; occupation; fluctuations in population, and too short an average of years. Many natives of the place may have derived their tubercles from parents, who were born and reared elsewhere, some years before; many, too, may owe their weakness to marriages of consanguinity, or to personal vices of constitution. So, too, the trade, and social status of the individual case must have a wide influence; for we should naturally expect more phthisis among a village of shoemakers, for example, than in a fishing town. Such hereditary, or other innate influences as there are, may vary much, on one side or the other, from year to year, from the emigration, or the moving of a certain per centage of floating population. And to compensate for these sources of error, a much longer series of years must be devoted to tabular returns—longer in proportion to the number and complexity of the causes involved.

Statistical results require to be controlled by new results before they can obtain the force of laws. The advocate of the numerical method is sometimes as one-sided as the specialist. He is ready to forget that figures are not brains, tables not perceptions, and that recorded observations do not give the power of observing. The statistician is but too often as fallacious and extravagant in his conclusions as those who rely exclusively on physical signs; both equally overlook the rational part of medicine.

Striking examples are not wanting, and we do not have to go further to seek them, than the pages of the contemporary periodicals of medicine.

One enthusiast recently proposed, in all gravity, to deliver all presentations by turning, as soon as the os was dilated. Another (T. J. Austin on *Paralysie Generale*, in the *Medical Times and Gazette*, Nov. 12, 1859, p. 486), says of the condition of the pupil in the general paralysis of the insane, "The iris is always affected; generally unequally in the two eyes. When the *right* pupil is most affected, the general tone of the delusion is *melancholic;* but when the *left*, usually *elated:*" whence he derives, by a brief chain of reasoning, the startling deduction that "the right *thalamus opticus* is the ganglion of natural painful emotion, and the left, of healthy, pleasurable emotion." And this result is based on the observation and autopsies of *twenty-six cases!!* Surely the fallacy of a too small number of observations requires no more striking illustration.

Nor are the results in large averages always more satisfactory. For example: an article, in the *Archives Generales de Medecine*, Juin, 1859, p. 691, et seq., based on some thousand observations, and the investigations of Dr. Adams, in the *Mass. Med. Communications*, Vol. IX., No. IV., 1858, of some seven hundred cases, exactly *contradict* each other in certain conclusions drawn from the statistics of vaccination.

"Of all dangers, a fallacious certainty is the greatest. A simple process of verification *à posteriori*, like the numerical method, never can be elevated to the dignity of a system, since it will be eternally true in medicine, that the problem is individual." We know that this method must be still more incompetent for the treatment of disease. And it requires, finally, such an amount of prolonged labor, that neither the life-time nor toil of any one person is adequate to it, but its statistics can only be drawn from the records of great Hospitals. Louis himself said of it, that nothing was more simple, and nothing more tedious.

Notwithstanding so many and so valid objections, the numerical method has not only been dignified with the name of a science, but actually exists, and can exhibit certain practical results not devoid of importance.

It would be very unfair to pass them by: and we will therefore speak, in conclusion, of the *Value* of Statistics in the Observation of Disease.

In a limited degree they have a value: far greater in some departments than in others. This value descends in accuracy by a progressive ratio, and in the following order: Mortality and Births; Hygiene; Etiology; Pathology; Therapeutics. The last is infinitely less certain than the first. And in accordance with our previous conclusions, we find the statistics reliable in proportion to the simplicity of the *data* from which they are calculated.

Even if genius could grasp the laws and causes of disease, observation would still be necessary to test their truth. We shall find in medical history that it is the detailers of facts alone, who escape oblivion. We see no examples in the history of science of any individual genius throwing itself far in advance of its contemporaries; but all attainment is the result of slow, and combined exertion. Faithful description, too, is always valuable, though the hypothesis which it seeks to establish may be absurd. And even if we settle nothing by our observations, the gradual accumulation of our facts may enable posterity to do so. A great mass of medical knowledge is, even now, only waiting for analysis. The authority of experience is but the attempt of an individual to generalize. And since no memory could recall enough observations to generalize just conclusions, we have need of the numerical method, which counts and compares individual facts. Nothing is here arbitrary or capricious, but simply mechanical. The correctness of the results is settled by a mathematical test, over which we have no control. Some laws require many more facts to establish them than others. But it is only when the objects contemplated are few, that individual varieties seem infinite: large masses of facts merge them in more general features. And in favor of very enlarged observations we have the testimony of Herschel that "It is only by condensing, simplifying and arranging the acquired knowledge of the past, that posterity can be enabled to avail themselves fully of the advanced stand-point from which they start."

To collect observations is a trade which must be learned, and not divined: nor can we trust others to observe for us. And to observe well, we must not observe hastily; but to re-examine an object as if presented the first time, is the only way, we are told, to rectify errors. But the professional man, though he carries on a certain inductive process in his mind, which results in establishing the conclusions of his daily experience, has no leisure for the requirements of statistics. Practitioners are all isolated; but general facts are required. And, in some respects, the present is a favorable time to get them. For the modern school of observation, with its more accurate methods of investigation by physical signs, the microscope and test-tube, has a tendency to discriminate more nicely between diseases. Differential diagnosis, one of the most essential elements in statistics, is therefore more exact, as well as of easier application. And where the diagnosis is very uniform, the limits of variability in our numbers is small. In such diseases as smallpox and tetanus, for example, but a small number of facts need be observed to settle definitely our diagnosis, or, in fact, our treatment.

Although the numerical method had been verbally recognized for ages, it was never practically tested and exemplified until the

skeptical mind of Louis had its attention drawn to it. From his youth until he had attained the age of thirty-three years, M. Louis studied and practised in Russia. Coming to Paris, he became a disciple of Broussais, whose theories were then in full tide of popularity. But soon doubting the accuracy of his results, he resolved to devote himself entirely to *observation*, for the purpose of trying to settle some of the many uncertainties in medical science. To obtain an extended field for his observations, he entered the Hospital of La Charité, as the clinical clerk of Chomel. He gave up all private business; and for *seven years* devoted himself to rigorous and impartial observation. Ridiculed at first; as soon as a numerical analysis of his facts could be made, he was admired and imitated by the French school.

Whatever we may think of his method, we can but admire his perseverance. There can be no doubt that he was the most careful, impartial and honest observer, whom our profession has seen. He was no specialist, and had no preconceived ideas to verify, or *à priori* views to establish. No one who has, will observe seven years, before reaching a conclusion. He studied all the functions during life, and examined all the organs after death. He analyzed his facts, and submitted them to a rigorous comparison with all analogous diseases. Special and characteristic symptoms, he held, could only be found by comparison.

Even Sydenham said, that the natural phenomena of disease, however minute, must all be noted. And to establish the natural history of disease the method of Louis holds out the most flattering promises. His two great series of observations on Phthisis, and on Typhoid Fever, have been long since well introduced by our native translators to the profession here. These two works, together with his researches on Yellow Fever, have not only established the fame of their author, and of the numerical method, but have aroused a hearty, coöperative observation throughout the medical world, which, although it may have unduly exalted statistics, has not been without good effects. For some very important empirical facts were early developed from the method of Louis.

Such are the quite positive conclusions that tubercles, in any organ of the body, after the age of fifteen years, involve their presence in the lungs: that chronic peritonitis, too, indicates pulmonary tubercles: that phthisis so often commences in the upper lobes, that we have been led to call those cases where the indications of its presence are found first at the base of the lungs, the *anomalous* development of tubercles. Again, how valuable is the knowledge that bronchitis and pneumonia oftener begin in the lower lobes, and that the former is mostly found in both lungs at once, but the latter only in one. No theory, and no speculation could ever have led to these results of numeration and averages. Such spontaneous creation of laws must have escaped our reason-

ing, because they do not agree with any of our preconceived opinions. Yet our ignorance of the conditions on which they depend is no bar to their utility. Although, then, the want of experiment, the presence of causal relations, from which we cannot disencumber facts, and the influence of various unknown powers modifying the phenomena of disease, must all moderate our expectations of the benefit of the numerical method, yet we have some useful results which cannot be done away with. So, too, in the typhoid affection, we find the characteristic lesion of Peyer's glands, the rose-colored eruption, the lassitude, the bleeding from the nose, the tympanites, as well as other pathognomonic symptoms, distinguishing it from typhus, marked out for us by the numerical method. not as invariable, but as present in the majority of cases.

So much accuracy has been conferred upon the student of medicine by physical signs, by chemistry, the microscope, and, to a certain extent, by the numerical method, that it has been asserted, that even Sydenham would have been but a smatterer beside the modern medical graduate. Yet, with all this, we venture to say that there have never been, and probably never will be in our profession, men of greater natural powers of observation, and of description, than Hippocrates, Sydenham, and Hunter, nor any who made better use of the light which their times afforded them.

Such a tendency has been awakened in later times to extend the numerical method to all branches of medical inquiry, that our periodical literature overflows with statistics, and every hospital annually tabulates the results of treatment. This is as it should be. And if we are often called to notice fallacies in the results of statistics, we can also record cases of the truly scientific employment of them. Such are to be found in the statistics of insanity, in the recent work of Messrs. Bucknill & Tuke on Psychological Medicine ; and in a very carefully tabulated Consideration of the Etiology of Continued Fever, by Charles Murchison, in the Medico-Chirurgical Transactions, Vol. XXIII. 1858. These leave nothing more to be asked from the careful employment of the numerical method, and carry its results as far as they are capable of going.

Louis himself mentions, as striking proof of the corrective tendency of the numerical method over the approximative one of simple experience, that, according to Corvisart, dilatation with thinning of the walls of the heart is common ; but that, on counting in his book, only one case was found. So, too, Bertin and Bouillaud make the same assertion. Yet, in forty-five cases of heart disease observed by Louis, no instance of it was seen. Laennec, also, says that ulcerations of the trachea are common in phthisis, but uncommon in those who have not tubercles ; yet, on numerical analysis, the very reverse is found to be the fact. But if this is sufficient to show the superiority of tabulated to remembered observa-

tion, it also indicates that morbid anatomy affords a much more profitable field for the numerical method, than the uncertainties of pathology can furnish. Both, indeed, must be as much superior, for its application, to therapeutics, as the science is superior to the art of medicine. For the additional uncertainties of treatment must still more prejudice its results.

Medicine has two provinces, to cure, and to prevent disease. The latter, though by far the most important, has ever been thought an inferior department, and been studied less than the former. Yet by how much is prevention nobler than cure! And it is in this department that most real progress has been, and can be made.

And here we may find the most profitable, as well as most certain application of the science of medical statistics. Etiology, as affected by vital and hygienic laws, has made more progress by the use of the numerical method, than in other ways.

The influence of a certain miasm, in producing intermittent fever, is so constant and invariable, that, like the contagion of smallpox, it requires no figures to prove it. Some few morbific agents are so constant as to need no calculation.

But it is the more doubtful ones that vital statistics have peculiarly enlightened. Such are the causes of typhoid and typhus fevers, of phthisis, of the spread and permanence of cholera and dysentery, &c. &c. The etiology of continued fever from bad drainage, cess-pools, and other poisonous influences, has been very well illustrated by the treatise of Dr. Murchison, before referred to. So, too, the returns of emigration, the reports of prisons, and the mortuary averages of over-crowded localities, have done much to indicate the way of prevention of typhus fever. Phthisis claims a much wider range of influences. But cholera and dysentery have, in later epidemics, been strikingly increased or diminished by certain local hygienic influences. In the localities most advanced in hygiene, their rate of annual mortality has steadily decreased. Vital statistics are here invaluable. And we can well believe that "if the attention of society were once given to these points, the saving of life would be such as would not only modify our tables of mortality, but affect the fortunes of nations."

It is very true, also, that the general tendency of the use of statistics is to discourage *à priori* conclusions, and that they tend to exactness, both in the observer, and the facts observed.

Yet we can hardly afford to substitute, in all cases, mathematics for logic; arithmetic for induction, or calculation for reason, as M. Louis has been accused of doing. Even La Place styles theory "common sense applied to calculation;" and adds that reasoning, logic and induction are as useful in medicine as numbers. Such a method has been styled *eclecticism* by its author (M. Double); and, as an opponent to Louis, he sums up his argument as follows:

"Individuality is an invariable element in pathology; therefore every exclusive theory is absurd in pathology, and every absolute method repugnant to therapeutics. Numerical calculations, open to many sources of fallacy, are in no degree applicable to therapeutics."

A good deal of force is to be found, in opposition to the fallacy and the merely approximate nature of the numerical method, in the certainty derived from that mathematical formula, known as the *calculus of probabilities.*

Since the use of a very large number of observations, in every case, is impracticable, how shall we know what value to attach to statistical conclusions derived from a limited series of facts only?

By the calculus of probabilities; which must be received as demonstrated authority by those who do not choose to study it mathematically. This method proves to us that the probability of a given event's happening does not exactly coincide with the actual number of times it has been observed to happen, but varies between limits somewhat greater and somewhat less than the number observed; and that these limits, moreover, are wider in proportion as the observations are few, and approach nearer as the observations become more numerous. We subjoin the mathematical formula, which determines these results, taken from the work of M. Gavarret, by Dr. Bartlett.*

To take an example, and one from Louis himself. He has given, as the result of his treatment of 140 cases of the typhoid fever, 52 deaths, and 88 recoveries: 52 + 88 = 140.

The mortality, therefore, might be supposed to be represented by $\frac{52}{140} = 0.37143$. Hence we should judge the mortality of typhoid, under the treatment of M. Louis, to be, approximatively, 37 deaths in 100 cases, or about .37 per cent.; a little more than one third.

Yet by using the calculations referred to, we shall find that the mortality may vary between the limits of

$$.37143 + .11550 = .48693, \quad \text{and}$$
$$.37143 - .11550 = .25593:$$

or approximatively, between .49 and .26 per cent.

In other words, that, by employing precisely the same treat-

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* If a represent the number of times that one of two events (call it A) has happened; b, the number of times that another event (B) has happened, and c represent the total number of observations collected, so that a plus b equal c: then the number which expresses the observed frequency of the event A, is not the true number, but merely an approximation to it, more or less close as the number of observations is greater or less. That number will, in any case, lie between

$$\frac{a}{c} + 2\sqrt{\frac{2.\ a.\ b.}{c^3}}$$

and

$$\frac{a}{c} - 2\sqrt{\frac{2.\ a.\ b.}{c^3}}$$

or at least, there are 212 chances to one in favor of its being comprised within those limits.

ment in a large number of cases of typhoid fever, we may lose from a quarter to one half of our patients; and not one third, as stated by M. Louis.

So, too, in comparing *any other* method of treatment with that of Louis, the aggregate sum of the conditions or circumstances remaining the same, it cannot be considered certain that the method is better or worse than his, unless the difference in the result exceeds these possible limits.

For to show the advantage of greater numbers of observations, we will take the following case :*

Let us suppose that 500 cases of a given disease have been subjected to a given treatment, with the result of 100 deaths, and 400 recoveries : and another 500 cases of the same disease have been subjected to a different treatment, with the result of 130 deaths, and 370 recoveries. In the first class, the ratio of mortality is as 20,000 to 100,000; in the second class, this ratio is as 26,000 to 100,000 ; the difference between the two being 6,000 in 100,000. An application to these numbers of the law of probabilities shows, that the limit of possible variation is equal to 7,508 in 100,000.

We cannot reasonably conclude, therefore, that the first method of treatment is better than the second, because the difference in the result *falls below* the limit of possible variation by the calculus of probabilities ; a variation which may be the effect of chance. The number of cases is not sufficient for the answer sought. But, by extending our observation to twice the number of similar cases, in which the ratio of mortality remains in each class the same, we find the following results : The limit of possible variation, ascertained by the calculation of probabilities, when applied to a thousand cases, instead of five hundred, sinks from 7,508 in 100,000, to 5,306 in 100,000; which is *surpassed by* the observed difference in the ratio of mortality, this being as 6,000 in 100,000. Here then we have a positive demonstration of the superiority of the first mode of treatment over the second; and this demonstration got solely by increasing the number of our observations.

These calculations, it should be remembered, have nothing to do with the nature of the facts observed, but solely with their number. As far as they can be carried into medical investigations, they are, therefore, invaluable. We have before attempted to show why they are not generally applicable. And it must be evident to all, that the great difficulty is in getting strictly comparable facts, and enough of them. For the mechanical exactness of the numerical method makes one suspicious of it, when applied to the notoriously imperfect science, and still more fallible art, of medicine. Comparable facts may be employed, in which the sum of

* Bartlett's Philosophy of Medical Science.

possible causes remains the same ; that is to say, which are comparable with regard to those influences under our control. If the degree of variableness of aggregates be limited by the calculation of probabilities, the individual facts composing the aggregates may be fixed enough to be comparable.

But the aggregate of possible causes must remain invariable; otherwise the whole calculation falls to the ground, or the law will be modified by the new element which has been introduced.* Thus, in 1824 and 1825, the number of legitimate births in France amounted to 1,817,572. Of these, 939,641 were male, and 877,931 were female. During the same period, the number of illegitimate births was 140,566. Of these, 71,661 were male, and 68,905 were female. Among the legitimate births, the proportion of males is as 51,697 to 100,000; while, among the illegitimate births, the proportion is only as 50,980 to 100,000. Now the difference might have amounted to 391 in 100,000 births, without surpassing the limits of the law of probabilities ; but it really amounts to 717 in 100,000 births. Some difference must exist, then, in the sum of the possible causes to explain this. And this difference can only be found in the fact of legitimacy, or illegitimacy. In the same way is to be ascertained the law of the average number of children born to each family. But here, also, changes in the physical, moral, or social condition of the people may alter the sum of possible causes.

Therefore, as a general deduction from the above, we have the following rule. Each series of relationships and phenomena must be fixed enough to be comparable; must consist of large numbers; the limits of variability must be determined by the calculation of probabilities; and the sum of possible causes must continue uniform. The law will be positive in proportion to the completeness of the above conditions.

Yet, with all these discouraging requisitions, the faculties of comparison and generalization still remain divine attributes, and those which proudly distinguish the human intellect from the instinctive and ever-uniform acts of the brute creation. If the vastness of the observations to establish a law must bear a fair proportion to the vastness of the circumstances controlling, or at least concomitant, under which the phenomena occur, yet we should consider that, in proportion to the complexity of the phenomena, is augmented the number of relations in which they *may* be surveyed and observed. All practical expedients and empirical rules are not to be neglected because they have not been rigorously defined and limited by the numerical method. For all common rules of the medical art have been ascertained and established by a series of observations of such vast extent as to compensate, in a

* Elisha Bartlett, op. cit.

great degree, for the absence of the other conditions of mathematical induction. The benefits of opium in pain; of mercury in secondary syphilis ; of quinine in intermittent fever, or of arsenic in the papular or squamous affections of the skin, have had the nearly unanimous testimony of all observers in their favor, for successive ages.

And the same thing is true of most of the generally admitted rules of practice. They rest upon the concurrent testimony of immense numbers of witnesses, and of an almost indefinite number of observations. Here, then, we have an instance of the value of simple experience. But it must be the experience of multitudes of observers, and of long series of years. Both here, and in the result of the calculation of probabilities, the uncertainty is rendered so unimportant as to be practically disregarded. The corrective influence of multiplied observation is, in these instances, analogous to that geometrical problem by which we can indefinitely approximate the sides of a polygon, inscribed within a circle, to the circumference of the circle, until it shall be impossible to distinguish the one from the other ; and the polygon has, to all intents and purposes, become a circle. Yet here, after all, we fail of the certainty of demonstration, and the result is, at best, *approximate.*

So the most careful methods which we may apply to the solution of medical problems are not only often utterly fallacious, but when most perfect, like this weaker part of geometry, are still inexact.

The inherent contradictions of medical science have been enhanced by the most opposite theories in all ages.

All the theorists say to the practitioner at the bed-side, "Do not try, but think; reason, argue, deduce!" Empirical Hunter said, "Do not think, but try!" So the modern disciples of the numerical method would say to us, "Neither think, nor try; but *calculate!*"

Meanwhile the patient dies. The average mortality, not only of the whole race, but of many acute diseases, remains unchanged century after century. Truly, when we consider the fallacies of medical science, its confusion, its contradictions, and its impracticable theories, as well as the weakness of the medical art, and the little which it really can do; and when we contrast, with these humiliating considerations, its high aims, and exalted calling among the other branches of human knowledge, we may well say, as has been said, with epigrammatic brevity, "*La médecine est la plus noble des professions, and le plus triste des métiers.*"

Yet the truly physiological and scientific practitioner, trained to the finest edge of acumen, and, above all, taught to observe everything, is the man for the times. For the study and analysis of phenomena, and their relationships, and not the discovery of any

general law like gravitation, marks those who are the Newtons of medicine. So did Hippocrates, Sydenham, and Hunter, Laennec, Andral, and Louis. And so should we all. The science of medicine wants facts; comparable, numerous, well observed, carefully arranged, minutely classified, and acutely analyzed. But little reward awaits those who collect them. He who devotes himself to the science of medicine must expect little sympathy from the mere votary of the art.

His reward lies in posterity, and the test of his conclusions must be in the future. No other agent but the lapse of time, can rightly estimate the varied elements which constitute the science and the art of medicine. This alone can finally arbitrate between the claims of statistics, and of the other methods of observation. So says Bacon:

"RECTE VERITAS TEMPORIS FILIA DICITUR, NON AUCTORITATIS."

Errata.

References, Page 2—
For "F. Oesterlu," read *F. Oesterlen.*

Page 3, line 2d—
For "qui j'en porte," read *que* j'en porte.

Page 9, line 35th—
For "casual," read *causal.*